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Private Health Insurance and Public Expenditures in Indonesia

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Abstract

Public and private financing of health care are often assumed to be substitutes, and many developing country governments rely on this assumption when formulating policies to encourage private insurance markets. This paper discusses a model in which private health insurance can increase public health care expenditures when access to complementary subsidized public safety net facilities is untargeted. The paper then tests this complementarity hypothesis against the conventional hypothesis of substitutability between public and private financing. Using household data from Indonesia, demand models of private and public health care visits are estimated. To control for adverse or positive selection in insurance coverage, fixed effects models are employed, exploiting individual-level shocks to health insurance coverage. Instrumental variables fixed effects models are estimated as well, and by modeling endogenous insurance in the various models, a lower bound on the effects of insurance on health care demand is derived.

Introduction

Public and private financing of health care are often assumed to be substitutes. Partly based on this assumption, a policy commonly pursued by governments is to encourage private insurance markets, with the stated intention of lessening the public health care budgetary burden. Along with encouraging private insurance, governments typically also provide public health care through a safety net of subsidized facilities. In this paper we argue theoretically and empirically that this combination of policies may result in private insurance expansions causing unanticipated increases in public health care spending. While the optimal insurance incentives in any setting will depend on many factors beyond public fiscal constraints, it is nevertheless important to understand the substitution and complementary effects induced by insurance policy.

This paper discusses a model of health care demand which incorporates two common features of health financing institutions in low-income countries: public health care subsidies are large and poorly targeted, and private insurance plans have highly limited benefits. Under these conditions, it is plausible that public and private financing of health insurance may actually be complements.

We test this complementarity hypothesis of the model against the conventional hypothesis of substitutability using two waves of household panel data from Indonesia. Separate public and private demand models of health care visits are estimated as a function of insurance status. To control for adverse or positive selection in insurance coverage, a fixed effects estimator is employed, exploiting individual-level shocks to employer-based health insurance coverage. Because fixed effects models may also be biased due to time-varying health shocks, we also estimate instrumental variables fixed effects models. In addition, by modeling the contrasting adverse and positive selection biases in these two models, we show that bounds on the true moral hazard effects of insurance can be derived even if both estimators are individually biased.

Background

Formal health insurance coverage is extremely limited in most countries of the world. By far the most common mechanism for risk-spreading in low-income countries is the availability of subsidized care at public clinics and hospitals, financed primarily via general revenues. Where user fees are charged, they tend to be only weakly means-tested. This arrangement can be sub-optimal for a number of reasons, and governments have sought to encourage the development of alternative health financing schemes.

Among formal health insurance options, the most prevalent tend to be public social security plans for civil servants. Rather than reimbursing private sector care, these plans typically administer a network of health care facilities that can be accessed only by beneficiaries. Reimbursement-based insurance is rare at the individual-level, where adverse selection has limited the spread of plans to select populations with narrowly defined coverage.

As an alternative to the above, a widely discussed strategy for expanding private health care financing is to mandate that private sector firms provide health benefits to employees. For example, in the 1990's the Indonesian government has mandated that all medium to large companies provide health insurance for their employees and their families. Given that the incidence of such benefit premiums is likely to fall primarily on the employees (see e.g., Gruber, 1997, for evidence in Chile), however, it is clear that with relatively low incomes, the scope of the health benefits is often extremely limited.

In this paper we focus on the two predominant forms of private employer-provided health benefits in Indonesia in the 1991-1993 period spanned by our data: (1) workplace health clinics, and (2) fee-for-service reimbursement plans. During this period total health care spending in Indonesia was estimated at about 2.1 percent of GDP. Government policy limited publicly financed health expenditures to just 0.6 percent of GDP, which is about 2.5 percent of government spending, or about $4.40 per person.

Substitution and Complementarity Hypotheses

The two insurance types analyzed in this paper can affect public and private demand through different mechanisms. The fee-for-service reimbursement type is the more straightforward of the two. While reimbursement lowers the cost of both public and private care, the relative prices between them remain unchanged. Even under a simple Cobb-Douglas utility model that does not allow complementarity in the utility function directly, standard moral hazard considerations predict complementarity arising from private financing. By further reducing the out-of-pocket cost at public facilities, this form of private insurance may raise the demand for health care at public clinics as well as private ones. In the absence of rigorous targeting of subsidies in public clinics, the private insurance will not crowd-out the public subsidy.

There are few direct studies of moral hazard effects of insurance in developing countries that have confronted the issue of endogenous insurance, but alternative evidence can be found from the user fee price elasticity literature. Although user fees in public clinics are already low, Gertler and Molyneaux (1996) found substantial negative price elasticities in Indonesia, in line with price elasticity findings elsewhere in low-income populations (for a review see Jimenez, 1995).

A subtler relationship with public care may hold in the case of workplace health clinics. Consider a simple two-period model of health care demand *x* in period *t* as a function of health care prices *P* and perceived health *H*, both of which enter negatively:

$$x_t = \pi_1 P_t + \pi_2 H_t$$

Let the initial period *t* health be drawn randomly, and model subsequent health as a function of lagged utilization and lagged health:

$$H_{t+1} = \delta_1 x_t + \delta_2 H_t$$

Now consider the demand effect of a permanent price increase at time *t*, due for example to loss of insurance benefits. Period *t* demand will decrease via the direct price effect: $\partial x_t / \partial P_t = \pi_1$. Period *t+1* demand will also decrease via the direct effect of the lower *t+1* price, but *t+1* demand will also be affected indirectly via the perceived health change induced by the time *t* utilization:

$$\frac{\partial x_{t+1}}{\partial P_{t+1}} + \frac{\partial x_{t+1}}{\partial P_t} = \pi_1 + \pi_1 \pi_2 \delta_1$$

There are two competing hypotheses regarding this net effect:

- *Dynamic substitution*: $*_1$>0. This can obtain if past utilization improves the stock of perceived health, thus decreasing current demand.
- *Dynamic complementarity*: $*_1$<0. This can obtain if past utilization offers new diagnosis information which decreases perceived health, or otherwise induces follow-up care.

In the case of workplace health care clinics, it is seldom that they offer a comprehensive range of services. Rather, they are more likely to offer diagnostic services which result in referral to another site, which may often be a subsidized public facility. It is an empirical question how common such referral may be, and whether the induced demand results in cost-savings from early diagnosis, or net cost increases.

This simple model obviously ignores important issues such as dynamic expectations. We have also not made explicit assumptions about information assymetries, such as those discussed by Arrow (1963). While assymetries could magnify any dynamic complementarity effects, they are not strictly necessary.

There is little empirical evidence on the relative importance of substitution versus complementarity. This study builds on earlier work by (Gertler and Sturm, 1997) which found evidence in support of substitution in Jamaica, estimating that private insurance would decrease public curative visits 45% and increase private ones by 37%. That study, however, was based on cross-sectional data, with insurance treated exogenously. The present study substantially expands the range and intepretation of empirical tests conducted, as outlined below, and finds evidence that strongly rejects results from OLS cross-sectional estimators.

Empirical Framework with Adverse and Positive Selection

The key empirical test of the substitution vs. complementarity hypotheses is whether private health insurance lowers or raises the utilization of subsidized public health care. A central difficulty in estimating the public demand elasticity of private insurance is the potential endogeneity of insurance. Furthermore, it is not a priori clear even in what direction this endogeneity may bias standard estimators. For example, a common hypothesis is that adverse selection exists in insurance markets, leading to over-estimation of the moral hazard effects of insurance in cross-sectional applications. In the United States this view may be supported by the fact the randomized insurance design in the RAND Health Insurance Experiment (Newhouse et al., 1993) found price-elasticities of demand that were in the low end of the range estimated from non-experimental data.

In many settings, however, positive selection may be more prominent than adverse selection. For example, in the United States, Cawley and Philipson (1999) find evidence supporting the existence of positive selection in life insurance markets, arising from the strong incentive which life insurers have to obtain information on individual health status. In employment-based health insurance markets similar incentives exist, and the ability to act on such incentives increases as labor law enforcement decreases. In developing countries, it is hypothesized that not only may employers screen potential employees based on health status, but it may be common for negative health shocks to result in job loss. In the absence of insurance continuation coverage regulations, negative health shocks can thus result in loss of insurance.

In order to help empirically distinguish between the effects of adverse versus positive selection on moral hazard estimates, we analyze a stylized linear model of health care demand *x* as a function of insurance *I* and health *H*. Assume the true model (with individual subscripts suppressed), where $\beta_1^* > 0$ and $\beta_2^* < 0$:

(1) $$x = \beta_1^* I + \beta_2^* H + \varepsilon^*$$

To illustrate the endogeneity issue we will assume that health *H* is unobserved.

First consider cross-sectional OLS estimation of (1) where we assume that omitted health is the only source of endogeneity:

(2) $$x = \beta_1^{OLS} I + \varepsilon^{OLS} \qquad \text{where} \quad \varepsilon^{OLS} = \beta_2^* H + \varepsilon^*$$

The direction of bias in the OLS estimator depends directly on the sign of the correlation between insurance and health: $\beta_1^{OLS} = \beta_1^* + \beta_2^* \left[\dfrac{\text{cov}(I,H)}{\text{var}(I)} \right]$. We can distinguish between two scenarios:

(i) *Adverse selection*: $\text{cov}(I,H) < 0$ which implies $\beta_1^{OLS} > \beta_1^*$.

(ii) *Positive selection*: $\text{cov}(I,H) > 0$ which implies $\beta_1^{OLS} < \beta_1^*$.

This OLS model is of course simplified in numerous ways, such as omitted preferences in the error term which are likely to be also correlated with insurance choice.

Next we consider a simple 2-period fixed effects model, in which time-invariant tastes, etc., are controlled by first differencing:

(3) $$x_2 - x_1 = \beta_1^{FE}(I_2 - I_1) + (\varepsilon_2^{FE} - \varepsilon_1^{FE}) \quad \text{where}$$

$$\varepsilon_2^{FE} - \varepsilon_1^{FE} = \beta_2^*(H_2 - H_1) + (\varepsilon_2^* - \varepsilon_1^*)$$

While fixed health endowments are eliminated from this model, bias may still arise if insurance changes over time are affected by health shocks. The potential bias in this fixed effects estimator is determined by: $\beta_1^{FE} = \beta_1^* + \beta_2^* \left[\dfrac{\text{cov}(\Delta I_t, \Delta H_t)}{\text{var}(\Delta I_t)} \right]$. Again we can distinguish between the cases of adverse selection, for example when illness shocks cause people to search for employment with insurance benefits, and positive selection, which may occur if illness shocks lead to job loss:

(iii) *Adverse selection*: $\text{cov}(\Delta I_t, \Delta H_t) < 0$ which implies $\beta_1^{FE} > \beta_1^*$.

(iv) *Positive selection*: $\text{cov}(\Delta I_t, \Delta H_t) > 0$ which implies $\beta_1^{FE} < \beta_1^*$.

While selection is still an issue in the fixed effects model, to the extent that employment changes in response to health shocks occur with lags, selection biases may be lessened. Thus we can further hypothesize that in the two cases:

(v) *Adverse selection:* $\beta_1^{OLS} > \beta_1^{FE} > \beta_1^*$.

(vi) *Positive selection*: $\beta_1^{OLS} < \beta_1^{FE} < \beta_1^*$.

While (v) and (vi) provides a useful result in general, if positive selection dominates adverse selection in the current example, then both the OLS and fixed estimators would be biased down, in favor of the substitution hypothesis between public and private financing.

Fortunately a third estimator offers a potential solution: an instrumental variables fixed effects model. We illustrate the model again in a simple two-period first differences framework, where the first differenced insurance is treated as endogenous. As a candidate instrument for the insurance innovation in the first stage regression, we follow Rosenzweig and Wolpin (1995) in considering the baseline insurance value:

(4) $\quad I_2 - I_1 = \gamma I_1 + \nu$

Baseline insurance will be appropriately excluded from the structural equation on the assumption that the original insurance decision is determined before the health innovation H_2-H_1 is revealed. More specifically, this will be satisfied in a health transition model $\Delta H_2 = \rho H_1 + \eta$ in which D=0 and health follows a random walk. If the exclusion restriction is invalid, then again we can represent the bias term:

$\beta_1^{IV} = \beta_1^* + \beta_2^* \left[\dfrac{\text{cov}(\Delta \hat{I}_t, \Delta H_t)}{\text{var}(\Delta I_t)} \right]$. By substituting the predicted value from (4) into this expression, note that the sign of the bias depends on the sign of the correlation between I_1 and H_2-H_1. If D<0, implying that health status displays regression to the mean, then this estimator will be biased. For example, if baseline healthier people are more likely to have baseline insurance (positive selection), but the good health regresses to the mean, then this health decrease could cause insurance loss, implying negative covariance and hence an upward bias:

(vii) *Adverse selection*: $\text{cov}(I_1, H_2 - H_1) > 0$ which implies $\beta_1^{IV} < \beta_1^*$.

(viii) *Positive selection*: $\text{cov}(I_1, H_2 - H_1) < 0$ which implies $\beta_1^{IV} > \beta_1^*$.

The important feature of results (vii) and (viii) is that in contrast to the OLS and fixed effects estimators, adverse selection in this case will induce a *downward* bias in the IV estimator, while positive selection will induce an *upward* bias. By observing that

(ix) *Adverse selection:* $\beta_1^{FE} > \beta_1^* > \beta_1^{IV}$.

(x) *Positive selection*: $\beta_1^{FE} < \beta_1^* < \beta_1^{IV}$.

we now reach our central result showing the bounds on the true moral hazard effect of insurance:

(xi) $\min[\beta_1^{FE}, \beta_1^{IV}] < \beta_1^* < \max[\beta_1^{FE}, \beta_1^{IV}]$

Thus for example if both the fixed effects and instrumental variables estimators yield positive estimates, even though neither estimator completely controls for the endogeneity of insurance, we may conclude that the substitution hypothesis can be rejected.

Two caveats merit mention. First, note that the IV result relies on the assumption that D<0. If instead D>0, and individuals are able to anticipate future health shocks that are unobservable to employers, then it is possible for adverse selection to again lead to upward bias of the IV estimator. We regard this possibility as unlikely, but the assumption is at least partially testable. Second, note that the relationships between the fixed effects and IV estimators under the positive selection hypothesis may be empirically similar to those caused by classicial measurement error in insurance status. Similarly, the relationships under the adverse selection hypothesis may be empirically similar to those caused by correlated measurement errors over time. Results (v) and (vi) related to the OLS estimator may prove useful in ruling out the measurement error hypothesis, however. For example, classical measurement error would predict $\left|\beta_1^{OLS}\right| > \left|\beta_1^{FE}\right|$, which may be inconsistent with the positive selection prediction in (vi).

Data

To test the complementarity hypothesis we use 1991 and 1993 waves from the Indonesian Resource Mobilization Study (IRMS) household survey. The survey was conducted by a team from RAND in conjunction with Indonesian researchers and international agencies. In the first wave a cluster sample of approximately 6000 household in two provinces was conducted in October through December 1991. The households were resurveyed in fall 1993, with approximately 17% attrition between the waves. The sampling frame was designed to exclude civil servants, thus virtually none of the study participants were covered by the social security health insurance for government employees. This survey is described more fully in Gertler et al. (1995).

We focus our study on the sub-sample of 3,567 men ages 20-50 in the survey. Table 1 provides summary statistics for this group, indicating that approximately 8 percent in each wave were covered by employment-based health benefits; non-employment-based insurance was negligible. Insurance rates in other demographic groups were too low to reliably study. For example, only 1 percent of women ages 20-50 had insurance. Because of the low insurance rates, we aggregate the two health benefit types discussed earlier into a single "insurance" indicator, and test their joint effects. Of those with benefits, almost half had access to private workplace clinics. There was considerable variation in reported health benefits over time, with almost half of the insureds changing status between the two survey waves. It is unclear what portion of this variation is due to measurement error, but to the extent that it is it would be likely to cause attenuation bias in the estimates of both public and private demand models.

As is common in household-based surveys, the measured indicators of health care utilization include the number of outpatient visits to each facility type in the previous four weeks, as well as out-of-pocket expenditures. Since the full social expenditure cost of visits was not measured, our empirical models estimate health care visit counts by type, rather than expenditures. While in the full sample approximately 55 percent of outpatient health care visits were to subsidized public clinics, in our adult male sub-sample this figure was approximately 44 percent. The median out-of-pocket cost of public health care visits was 600 Rupiah in 1993, as compared to 1750 Rupiah for private visits. For reference, the median household monthly per capita expenditure was 39,429, implying that the median subsidized public visit cost 1.5 percent of that amount.

Estimation and Results

Direct tests of adverse or positive selection are reported in Table 2 via logit regression results of associations between observed health and insurance status. Related Indonesian research (Dow et al., 1997) has found activities of daily living to be more consistent indicators of health than general health status; both are reported here. The only indicator appearing significantly related to insurance cross-sectionally is ease in carrying heavy loads, which is negatively correlated with insurance, implying mild evidence of adverse selection. However, none of the indicators are significant in the individual-level OLS fixed effects models. This may indicate little selection, or more plausibly it may indicate the difficulty in measuring relevant health attributes in our household survey indicators.

Table 3 reports pooled OLS models of counts of public and private visits. Although the evidence is mixed across specifications, the results appear to indicate support for the substitution hypothesis that private insurance leads to increased private health care demand but reduced public visits.

Given that the visit counts are discrete variables with a large mass at zero that declines steadily down to 10, linear OLS may be a poor model for estimating insurance effects. The OLS model will yield simple conditional mean estimates: $=E[visits|insured, Z] – E[visits|not insured, Z]. Not only does OLS fail to capture the non-linearities in the true relationships, but it can generate negative visit count predictions, and standard errors will be under-stated due to heteroscedasticity.

Unfortunately, the alternative count data models, such as poisson and negative binomial, also have drawbacks in the present application. Aside from practical difficulties in accounting for survey design issues such as weights and clustering in the fixed effects count data models, we are not aware of existing applications of instrumental variables fixed effects count data models. In the interests of comparability in estimating the adverse and positive selection bounds, using a single functional form structure would be preferable, and the main tables below report OLS based models.

However, to address heteroscedasticity, all OLS models were estimated with ex-post Huber standard error corrections (survey clustering and sampling weights were also accounted for). Negative visit count predictions are not a direct concern given our inference needs, and this issue has been ignored. To investigate the sensitivity of results to alternatively estimating poisson models of visit counts, Table 6 presents parallel OLS and poisson models, omitting survey design corrections. To compare magnitudes of inferences we can transform the poisson coefficient into a marginal effect equivalent to the OLS coefficient by multiplying the poisson coefficient by the mean visit counts (public 0.12, private 0.15). While the point estimates of the marginal effects differ by up to 20% in the linear and non-linear models, the directions of the differences are not systematic, and the statistical and substantive significance of the results is unaffected by model choice in these specifications. Extensions to negative binomial models yielded similar results, and the comparisons of fixed effects OLS models to fixed effects poisson models in Table 6 again show comparable results. Thus it appears that in this particular application, substantives inferences are largely unaffected by the choice of functional form among these models.

The results of the fixed effects and instrumental variables fixed effects models crucial to the bounding analysis are reported in Table 4. The first stage explanatory power of baseline insurance as an instrument is reported in Table 5, and indicates strong significance, implying that finite sample bias may not be a first order concern.

Focusing first on private demand in Table 4, insurance appears to have a strong positive effect in both the fixed effects and IV models. Consistent with the earlier results directly examining the relationship between insurance and health, controlling for observed changes in health status have little affect on the insurance impact. However, in comparing across the different models, there is a pattern consistent with results (vi) and (x): $\beta_1^{OLS} < \beta_1^{FE} < \beta_1^{IV}$, suggesting strong positive selection. If correct, the point estimates on the bounds of the true moral hazard effect of private insurance on private health care are to increase outpatient visits by between 0.12 and 0.21 per month, or between 80% and 140% per month. In the absence of information on the exact designs of the insurance benefits it is difficult to translate this magnitude into a price-elasticity, but it would likely be on the high range of estimates in the literature.

Lastly, we focus on the cross-effect of private insurance on public demand. These estimates also suggest the importance of positive selection, with $\beta_1^{OLS} < \beta_1^{FE} < \beta_1^{IV}$. Based on the point estimates, the bounds indicate a true effect of between 0.01 and .11 visits per month, which would be consistent with complementarity. The standard errors are sufficiently large, however, that a 95% confidence interval on the lower bound extends to approximately –0.05 visits per month. Thus although the point estimates on the complementarity test appear positive, it is not possible to reject substitution at conventional significance levels in this data sample. If the null hypothesis were instead stated to be a zero effect though, the data would likewise not reject that null hypothesis, and thus would not support the conventional wisdom that private insurance will significantly relieve public health care budgets.

Discussion

The complementarity hypothesis and selection bounding framework developed in this paper may be applicable to a wide variety of settings. Although these data are not sufficiently rich to precisely estimate complementarity or substitution of public care in Indonesia, they do appear to indicate that inferences from cross-sectional observational insurance study designs may be significantly flawed.

In addition to attention to selection bounding, future studies may benefit from careful attention to a variety of measurement issues. First, although the result patterns are not consistent with attenuation bias from classical measurement error, it is possible that results were biased from systematic mis-classification of public and private visits. Second, more detailed information on insurance benefit design would significantly aid interpretation. Third, while studying outpatient visits is a useful first step, a fuller understanding of the effects of private insurance on public demand will require measurement of visit intensity and inpatient expenditures.

From a public budgeting perspective, the results thus far certainly do not support the encouragement of private insurance markets simply as a tool to reduce fiscal burden. From a welfare pespective there are many other potential reasons for encouraging private insurance markets; a body of empirical work modeling these fuller insurance effects on demand preferences would be of great interest.

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Table 1: Summary statistics

Variables		Mean (s.d.)	
Age (years)		34.1917	
		(0.1500)	
Education(years)		5.4152	
		(0.0761)	
Ln PC expenditure		10.4058	
		(.0122)	
Urban(%)		0.2455	
		(0.0074)	
	1991	1993	Difference (1993-1991)
Health care demand (mean # of visits)			
Public	.1170	.1240	.0070
	(.0086)	(.0086)	(.0113)
Private	.1646	.1372	-.0274
	(.0101)	(.0093)	(.0133)
Health insurance(%)	0.0851	0.0781	-.0070
	(0.0051)	(0.0048)	(.0051)
Health status(%)			
Very healthy	0.2677	0.3845	.1168
	(0.0078)	(0.0086)	(.0111)
Somewhat healthy	0.5283	0.4261	-.1022
	(0.0088)	(0.0087)	(.0122)
Easily carry heavy load 20m	0.8791	0.8736	-.0056
	(0.0059)	(0.0059)	(.0073)
Easily sweep floor	0.8922	0.8856	-.0066
	(0.0056)	(0.0057)	(.0070)
Easily walk 5 kms	0.7695	0.8452	.0757
	(0.0075)	(0.0065)	(.0090)
Easily take water from well	0.8885	0.8822	-.0062
	(0.0057)	(0.0058)	(.0071)

Notes: Means and standard errors weighted by sample weights. Sample has 3567 observations, for males ages 21-50.

Table 2: Insurance correlates (for males ages 21-50)

	Logit models (1991)			Logit models (1993)			Logit (pooling 1991 & 1993)			Fixed-effect OLS		
	Base1	Base2	Extended	Base1	Base2	Extended	Base1	Base2	Extended	Base1	Base2	Extended
Age (years)	----	----	-0.0309 ***	----	----	-0.0106	----	----	-0.0215 ***	----	----	0.0011 *
			(0.0095)			(0.0097)			(0.0078)			(0.0006)
Education (years)	----	----	0.1007 ***	----	----	0.1285 ***	----	----	0.1135 ***	----	----	0.0014
			(0.0207)			(0.0203)			(0.0172)			(0.0014)
Urban	----	----	0.4326	----	----	0.4193	----	----	0.4505 *	----	----	-0.0049
			(0.2782)			(0.2766)			(0.2557)			(0.0182)
Ln Per capita expend.	----	----	0.6530 **	----	----	0.5324 **	----	----	0.5858 ***	----	----	-0.0082
			(0.3028)			(0.2431)			(0.2105)			(0.0450)
General health:												
very healthy	----	0.3392	-0.2697	----	0.7131 **	0.0928	----	0.4965 **	-0.1039	----	-0.0239 **	-0.0200 *
		(0.3872)	(0.3670)		(0.3375)	(0.3541)		(0.2423)	(0.2381)		(0.0109)	(0.0106)
Somewhat healthy	----	0.2826	-0.0111	----	0.6599 *	-0.0119	----	0.4568 **	0.0272	----	-0.0011	-0.0025
		(0.2910)	(0.2991)		(0.3538)	(0.3524)		(0.2317)	(0.2333)		(0.0096)	(0.0096)
Carry heavy load 20m	-0.1769	-0.2866	-0.1913	1.6606 ***	1.3970 **	2.0455 ***	0.4146	0.2536	0.3719	0.0234	0.0262	0.0236
	(0.4225)	(0.4325)	(0.6005)	(0.5304)	(0.5928)	(0.5618)	(0.3523)	(0.3636)	(0.5288)	(0.0295)	(0.0294)	(0.0298)
Sweep floor	-1.4521 ***	-1.4365 ***	-1.3537 **	-1.7350 ***	-1.8733 ***	-1.6806 ***	-1.3940 ***	-1.4108 ***	-1.1357 **	-0.0190	-0.0153	-0.0122
	(0.4623)	(0.4599)	(0.6397)	(0.4873)	(0.4716)	(0.5214)	(0.3130)	(0.3093)	(0.4683)	(0.0358)	(0.0359)	(0.0367)
Walk 5 kms	0.5322 *	0.4345	0.2272	0.3766	0.1296	0.4572	0.4545 *	0.3046	0.1725	-0.0037	0.0011	0.0036
	(0.2945)	(0.3265)	(0.3381)	(0.5644)	(0.6036)	(0.5489)	(0.2497)	(0.2683)	(0.2676)	(0.0116)	(0.0111)	(0.0112)
Take water from well	0.6265	0.5301	1.2485	-0.8141 *	-0.8135 *	-1.0713 **	0.0343	-0.0875	0.4296	0.0224	0.0204	0.0141
	(0.5747)	(0.5993)	(0.7789)	(0.4394)	(0.4546)	(0.5159)	(0.3783)	(0.3923)	(0.5885)	(0.0373)	(0.0373)	(0.0363)
Intercept	-1.9168 ***	-1.9162 ***	-9.7977 ***	-2.0095 ***	-2.0148 ***	-8.8226 ***	-1.9646 ***	-1.9655 ***	-9.1817 ***	-0.0066	-0.0042	0.0803
	(0.2247)	(0.2249)	(3.6210)	(0.1920)	(0.1922)	(2.8919)	(0.1798)	(0.1797)	(2.5439)	(0.0059)	(0.0058)	(0.5012)
F(marital, district and interaction of district and expenditure)	----	----	98.94	----	----	119.84	----	----	132.29	----	----	1.31
			(0.0000)			(0.0000)			(0.0000)			(0.1639)
Observations	3567	3567	3540	3567	3567	3540	7134	7134	7080	3567	3567	3567
Log likelihood	-1029.06	-1028.32	-866.12	-970.00	-967.51	-770.50	-2001.63	-1998.75	-1655.25	----	----	----
R-squared	----	----	----	----	----	----	----	----	----	0.0012	0.0036	0.0115

Note: Clustered robust standard errors are in parentheses; Means and standard errors are weighted by sample weights;
* : Statistically significant at 0.10 level;
** : Statistically significant at 0.05 level;
***: Statistically significant at 0.01 level.

Table 3: Cross-sectional models: effect of health insurance on demand (for males ages 21-50)

	1991				1993			
	Public health care demand		Private health care demand		Public health care demand		Private health care demand	
Variables	Base model	Extended model	Base model	Extended model	Base model	Extended model	Base model	Extended model
Health insurance	-0.0011 (0.0352)	0.0360 (0.0362)	0.1049 ** (0.0458)	0.1510 *** (0.0469)	-0.0865 *** (0.0193)	-0.0402 ** (0.0180)	0.0169 (0.0344)	0.0655 * (0.0392)
Age (years)		0.0008 (0.0011)		-0.0003 (0.0013)		0.0029 ** (0.0012)		0.0036 *** (0.0014)
Education (years)		0.0010 (0.0028)		-0.0013 (0.0027)		0.0033 (0.0025)		0.0015 (0.0026)
Urban		0.0065 (0.0275)		-0.0067 (0.0229)		-0.0374 ** (0.0186)		-0.0079 (0.0228)
Ln Per capita expend.		-0.0023 (0.0192)		0.0463 (0.0455)		0.0271 (0.0340)		0.0182 (0.0314)
General health: very healthy		0.19458 *** (0.0506)		0.1945 *** (0.0701)		0.1945 *** (0.0575)		0.1945 *** (0.0657)
Somewhat healthy		-0.1821 *** (0.0480)		-0.2600 *** (0.0673)		-0.2854 *** (0.0581)		-0.2560 *** (0.0660)
Carry heavy load 20m		-0.1443 (0.1645)		-0.0334 (0.1446)		-0.0297 (0.1584)		-0.5094 (0.3413)
Sweep floor		0.6323 *** (0.1923)		0.6518 *** (0.2050)		0.3198 (0.2191)		0.1478 (0.2172)
Walk 5 kms		-0.0792 * (0.0414)		-0.1119 ** (0.0437)		-0.1539 (0.0939)		-0.0656 (0.0763)
Take water from well		-0.2433 (0.1914)		-0.2776 (0.1822)		0.1002 (0.2210)		0.6061 ** (0.3066)
Intercept	0.1171 ** (0.0109)	0.1003 (0.2107)	0.1557 ** (0.0127)	-0.3856 (0.5119)	0.1307 *** (0.0108)	-0.3308 (0.3853)	0.1359 *** (0.0108)	-0.3602 (0.3523)
Adjusted R_squared	0.0000	0.0550	0.0026	0.0716	0.0022	0.0637	0.0001	0.0617
F(marital, district and interaction of district and expenditure)		2.63 (0.0002)		5.13 (0.0000)		2.96 (0.0000)		6.26 (0.0000)

Note: Sample size is 3567 for both 1991 and 1993; Robust standard errors are in parenthesis; P-values in brackets below F statistics; Means and standard errors weighted by sample weights; SMSDI 91 area is used as the cluster indicator;
* : Statistically significant at 0.10 level;
** : Statistically significant at 0.05 level;
***: Statistically significant at 0.01 level.

Table 4: OLS first difference and 2SLS first difference models: effect of health insurance on demand (for males ages 21-50)

	FE OLS models				IV_FE OLS models			
	Public health care demand		Private health care demand		Public health care demand		Private health care demand	
Variables	Base model	Extended model	Base model	Extended model	Base model	Extended model	Base model	Extended model
First-Differenced								
Health insurance	0.0217	0.0150	0.1327 ***	0.1231 ***	0.1439 **	0.1055	0.2229 ***	0.2070 ***
	(0.0332)	(0.0339)	(0.0485)	(0.0471)	(0.0695)	(0.0649)	(0.0828)	(0.0774)
General health:								
Very healthy		-0.2242 ***		-0.2716 ***		-0.2352 ***		-0.2465 ***
		(0.0387)		(0.0584)		(0.0340)		(0.0406)
Somewhat healthy		-0.2207 ***		-0.2283 ***		-0.2335 ***		-0.2055 ***
		(0.0402)		(0.0619)		(0.0323)		(0.0385)
Carry heavy load 20m		0.0595		0.0832		-0.0115		0.0509
		(0.1059)		(0.1381)		(0.0724)		(0.0864)
Sweep floor		0.1704		0.2928		0.2138 **		0.3385 ***
		(0.1508)		(0.1918)		(0.0926)		(0.1105)
Walk 5 km.		-0.0744		-0.1591 ***		-0.0671 **		-0.1671 ***
		(0.0459)		(0.0572)		(0.0324)		(0.0386)
Take water from well		0.0437		-0.0102		0.0638 **		-0.0564
		(0.1394)		(0.1760)		(0.0945)		(0.1128)
1991 Baseline								
1991 ln PCE		0.0140		-0.0190		0.0074		-0.0224
		(0.0387)		(0.0665)		(0.0637)		(0.0760)
1991 Age (years)		0.0022		0.0036 **		0.0023		0.0029
		(0.0014)		(0.0018)		(0.0015)		(0.0018)
1991 Education (yrs)		0.0006		0.0019		0.0003		0.0010
		(0.0032)		(0.0031)		(0.0031)		(0.0037)
1991 Urban		-0.0528 **		-0.0056		-0.0575 *		-0.0072
		(0.0234)		(0.0300)		(0.0306)		(0.0365)
Intercept	0.0072	-0.2775	-0.0264 *	-0.0928	0.0087	-0.2282	-0.0202	0.0089
	(0.0115)	(0.4356)	(0.0140)	(0.7277)	(0.0109)	(0.7083)	(0.0130)	(0.8453)
Adjusted R-squared	0.0001	0.0297	0.0025	0.0369	--	0.0231	0.0008	0.0244
F(**Baseline**: marrital status, district, interaction of district and lnPCE)		1.98		2.82		1.41		1.05
		(.0065)		(0.0000)		(0.0936)		(0.4000)

Note: Sample size is 3567 for both FE and IV_FE models; Robust standard errors are in parenthesis; P-values in brackets below F statistics; For FE models, means and standard errors weighted by sample weights; SMSDI 91 area is used as the cluster indicator;
* : Statistically significant at 0.10 level;
** : Statistically significant at 0.05 level;
***: Statistically significant at 0.01 level.

Table 5: First stage regresions in 2SLS first differenced models

	Base model	Extended model
Health insurance	-.5706 ***	-.6375 ***
	(0.0475)	(0.0439)
Intercept	.0415 ***	-.7273 *
	(0.0049)	(0.3928)
F(health insurance)	144.12	211.00
	(0.0000)	(0.0000)

Note: Sample size is 3567 for males ages 20-50. Robust standard errors are in parenthesis; P-values in brackets below F statistics; The extended model include age,education, marital status, 1991 ln per capita expenditure, urban, district, interaction of district and 1991 ln per capita expenditure, change of health status, and health insurance; Means and standard errors weighted by sample weights; SMSDI 91 area is used as the cluster indicator;
* : Statistically significant at 0.10 level;
** : Statistically significant at 0.05 level;
***: Statistically significant at 0.01 level.

Table 6: Comparing functional form, without survey design (for males ages 21-50)

	OLS		Poisson		Fixed-effect OLS		Fixed-effect Poisson	
	Public	**Private**	**Public**	**Private**	**Public**	**Private**	**Public**	**Private**
Health insurance	-0.0440 **	0.0634 **	-0.4289 ***	0.3495 ***	0.0109	0.1286 ***	0.1158	0.7665 ***
	(0.0219)	(0.0250)	(0.1499)	(0.0956)	(0.0382)	(0.0455)	(0.2656)	(0.2171)
	(0.0207)	(0.0309)	(0.2406)	(0.1487)	(0.0364)	(0.0519)	(---)	(---)
Year	0.0076	-0.0212	0.0617	-0.1358 **	----	----	0.0652	-0.1201 **
	(0.0119)	(0.0136)	(0.0676)	(0.0600)			(0.0677)	(0.0606)
	(0.0117)	(0.0133)	(0.0954)	(0.0851)			(---)	(---)
Intercept	0.1225 ***	0.1618 ***	-2.1003 ***	-1.8235 ***	0.0079	-0.0208	----	----
	(0.0086)	(0.0098)	(0.0493)	(0.0424)	(0.0109)	(0.0130)		
	(0.0109)	(0.0121)	(0.0899)	(0.0745)	(0.0117)	(0.0132)		

Note: Observations is 7134 for all models except for the fixed-effect Poisson models due to dropped groups with all zero outcomes. There are 1028 observations for the public health care demand model with fixed-effect Poisson, 1262 observations for the private health care demand model with fixed-effect Poisson; Standard errors are in the first parenthesis; Robust standard errors with cluster are in the second parenthesis; SMSDI 91 area is used as the cluster indicator;
* : Statistically significant at 0.10 level;
** : Statistically significant at 0.05 level;
***: Statistically significant at 0.01 level.